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RE-INTRODUCTION OF ETHER INTO ENGLAND.

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My student life in Europe in 1858–59 taught me that English and Continental surgeons were generally unacquainted with ether and its administration as an anæsthetic, to such an extent as to readily explain their adoption and continued use of chloroform, notwithstanding its frequent fatal effects. Hence, I have from that time thought it would be doing good service to show our medical brethren, the other side of the water, our method of giving ether, and thus, perhaps, give them the confidence in it which we have here in Boston, where it was first used and has never been superseded. I had opportunity of doing this during my recent visit to England to attend the meeting of the International Ophthalmological Congress in London, Aug. 1–3, 1872. This Congress was composed of some 130 delegates, from South and North America, Russia, Germany, Italy, Spain, France, Holland and Great Britain, many of whom, especially the London men, were not only ophthalmic but also general surgeons. As they gathered at the several hospitals to witness operations on the eye by their London confrères, I naturally had the best opportunity of exhibiting to a large number of those unacquainted with it, the administration of ether, and thus establish their confidence in it, not only by what I said, but by what they could see for themselves. The Ophthalmic Congress was opened by my reading a paper on the "Value of Ether in Ophthalmic Surgery," which was published, with the remarks it elicited, in the *Lancet* of Aug. 17th, 1872. I showed the ether I carried with me from Boston, and offered to administer it for any gentlemen who desired it given for their operations. This resulted in my exhibiting ether in the various hospitals seventeen times during my brief stay in London. I should have continued doing so, but I desired to leave some of the anæsthetic, as I did at Guy's and the Royal London Ophthalmic Hospital, in order that the surgical staff might use it for themselves as they saw me do. The editorial remarks in the same number of the *Lancet* explain when and where I gave ether and for what operations. They are as follows:—

"We print to-day a paper read by Dr. B. Joy Jeffries before the Ophthalmological Congress on the use of ether as an anæsthetic. The reaction that has set in in favor of ether during the last few years is very remarkable, and it will be seen that Dr. Jeffries claims for it an absolute immunity from danger to life. During his stay in London, Dr. Jeffries administered ether, on the 29th of July, at the London Hospital, while Mr. Hutchinson excised a knee-joint; on the 30th, at King's College Hospital, while Mr. Soelberg Wells performed iridectomy and operated for strabismus; on the 2d of August, at the Royal London Ophthalmic Hospital, during an iridectomy by Mr. Critchett, and a flap extraction by Mr. Bowman; on the same day, at St. George's, during a double iridectomy, a single iridectomy, and a double extraction of cataract, by Mr. Brudenell Carter; on the 5th of August, at the Royal London Ophthalmic, during a removal of prolapsed vitreous by Mr. Streatfield, and a strabismus operation by Mr. Hutchinson; on the same day, at Guy's, during a double iridectomy for glaucoma, and during the removal of a foreign body from the anterior chamber by Mr. Bader; on the 6th of August, at the Royal London Ophthalmic, during the extraction of cataract (two patients), an enucleation of the globe, and a strabismus operation by Mr. Critchett, and a flap extraction of cataract by Mr. Bowman.

"In all these cases, Dr. Jeffries administered the ether in the manner set forth in his paper, and saturated his towel with it

with a freedom that at first sight seemed almost startling. In all the cases, the results were very good, and all the operators expressed themselves as being fully satisfied with the insensibility and muscular relaxation produced. If, indeed, it be true, as maintained by Dr. Jeffries, that the surgical use of ether cannot kill, it will have a strong claim to supersede chloroform alike in ophthalmic and in general surgery."

My experience in England, I mean what I saw and heard, proved conclusively to me that, whatever may have been the reasons in the past, at present, English and Continental surgeons do not use ether, principally because they are unacquainted with it, and rank it with chloroform, of which very many have a wholesome dread. Not being in any way an *etherist*, I mean as in England a *chloroformist*, I entered into no crusade against chloroform, but endeavored to show how simply and readily ether could be administered, and how perfectly harmless it was in any case where an anæsthetic was in place. It should be remembered that but very few of those who saw me give ether were not thoroughly convinced, from experience or otherwise, that ether was not a *practical anæsthetic*. I did not, and do not here, discuss the reasons for this. But I was as thoroughly determined to convince them through their own senses, to regard ether as I do. Truth will finally come out, and I foresaw the day when our English brethren, awakened to the danger of chloroform and the safety of ether, would naturally blame us Bostonians especially for not having enlightened them in spite of themselves. I found only incredulity to overcome in introducing ether into the London hospitals, and I will give the gentlemen there the credit of first, very properly, not believing till they had seen for themselves, and, secondly, frankly acknowledging they had not understood or appreciated ether as a *practical anæsthetic*. Before I left London its administration had been followed up as I taught them, and, of course, with the same success.

It is rather curious that the danger of chloroform had prevented ether having been properly administered. I mean that it was equally feared with chloroform, and hence not enough given at once in the commencement. Public opinion and public ignorance greatly hamper medical men in England as elsewhere in the world. When I asked the object of measuring the chloroform used, I was told, with a smile, "for the benefit of the coroner's jury." This explained to me the looks and expressions of astonishment when I lavishly poured out ether on my towel cone, which I purposely did to prove there need be no fear of the anæsthetic, the important point being how much the patient got, not how much was in the sponge or other apparatus.

As may be well imagined, I had to answer many questions from the English and foreign gentlemen grouped about me. To the very frequent one as to relative expense, I replied by showing them that if ether was manufactured and sold only as cheaply as in the United States, and not wasted in administration, its cost was little if any above that of chloroform. Politeness, of course, prevented my adding what was naturally in my mind, namely, that probably any gentleman who had had one fatal case from chloroform would gladly deduct from his fee any difference in price of the anæsthetic to avoid another.

Till my audiences were convinced by their own observation, I was repeatedly questioned, with more or less of incredulity, as to the time required to etherize a patient ready for operation. This, I found, was universally thought to be so great as to practically exclude the anæsthetic in hospital practice, where in the amphitheatre perhaps a dozen operations must rapidly succeed each other, one surgeon waiting impatiently for the preceding one to finish. Now, in reality, when administering ether in London, the question of time never once entered my head, and I never hurried with the anæsthetic, yet several times I turned to the operator within fifty to one hundred seconds, and told them the patient was ready. In looking up at my audience, the comparison of watches amused me as much as it surprised them. I judge the question of time was pretty definitely answered.

As to the relative disagreeabilities of ether and chloroform I frankly told them I was not in position to judge, for I had never administered the latter and was not an etherist; in fact, we had, and needed none among us, since surgeons in America gave ether themselves when operating, or it was exhibited by a surgeon assisting, or by a medical student, experience having taught us that it was not a dangerous anæsthetic like chloroform. I saw there was evident surprise on finding, from inquiry, that I was simply an ophthalmic surgeon and not in any way an etherist, as they have chloroformists. It was a good argument in favor of ether.

My previous and recent experience both showed me there was a strong and firmly-rooted incredulity in England and on the

Continent as to the *practical* value of ether as an anæsthetic, and in England especially as to its safety as compared with chloroform. Now, with all due deference, I really feel that English surgeons are to blame for this. My only argument in proof will be to quote the words of Dr. Benjamin W. Richardson, at our last interview in London, which were, "Why, Doctor, I repeatedly told them, years ago, that ether was safer than chloroform."

In our talk at this interview over the relative value, &c. of the several anæsthetics, Dr. Richardson, in explanation, called my attention to what he had recently said and written, the subject being then quite fresh in his mind, as but the previous day he had spoken upon these very points before the "British Association" meeting at Brighton. I therefore take the liberty of quoting from his "Report on the Physiological Action of Organic Chemical Compounds" in 1871. He says:—

"I have taken occasion several times to observe the effect of narcotic vapors on the minute circulation of the blood. I prefer to use the term 'minute circulation' because it embraces the minute arterial and venous, as well as the capillary circulation.

"The first fact I would notice as common to the action of all vapors used is, that no obvious change in the physical character of the blood-corpuscles, red or white, was ever observable; neither was there any noticeable difference in the relationships of the red and white corpuscles to each other. The red corpuscles held their ways so long as there was motion in the centre of the blood-streams, while the white ones rolled along by the sides of vessels in the same manner as they did before the narcotism.

"Another fact common to the action of all the vapors used was, that the first sign of arrested movement of the circulation commenced in every case on the venous side of the circulation, and consisted of a sort of pulsation or to-and-fro movement of the current through the vein; soon upon this the venous current became obviously slower and the vein dilated, while the arterial current remained, often for a long time, unchanged.

"To sum up, if my observations be correct, the action on the systemic circulation of the narcotic vapors named was seen to be primarily on the venous current or, I should more correctly say, was primarily manifested in the retardation of the venous current, secondly in the capillary, and finally in the arterial current. During recovery, moreover, the return of a steady onward current was manifested in the veins before it was restored in the capillary channels. This order of events coincides purely with the order of phenomena of death under the influence of narcotic vapors, as observed both in man and the lower animals. It is, I think, the invariable fact that the right side of the heart in such fatal cases is the first to cease its action, and in animals, when the heart is exposed to the air soon after death, the right side is the first to recommence action. From these facts the inference, I think, is clear that the arrest of the circulation begins, during the narcotism, in the retardation of the venous current, secondly in the capillary, and lastly in the arterial current.

"The changes named above were common to the action of all the narcotics named; but there were some striking changes peculiar to the substances themselves to which I must refer. The peculiarities were traceable, as it seems to me, to the weight, the solubility, and the chemical composition of the substance that was employed to produce the narcotic state.

"When the substance was very light, of low boiling-point, and insoluble, the effect of arrest of the circulation was most rapidly developed, and at the same time was most rapidly removed. Thus hydramyle, the lightest, the first to boil on elevation of temperature, and the most insoluble, produced the quickest arrest of the venous current; but from its influence the animal was equally quick to recover, the general signs of recovery being secondary to the local return of the circulation.

"When the substance was light and of low boiling-point, but comparatively soluble in blood, the time required to produce the slowing of the venous circulation was prolonged after the insensibility of the animal was complete; after even respiration had stopped, the extreme changes in the circulation were slowly developed; and although the insensibility might be deep and continuous, like to death itself, the actual temporary arrest of the arterial current was imperfectly pronounced. Absolute ether, which has a very low specific weight (720) and a very low boiling point (94° F.), but which is soluble in blood to the extent of not less than eleven parts in the hundred, produced perfectly all the effects immediately named above. When the substance inhaled was comparatively heavier, of a higher boiling-point, insoluble, and contained as one of its elements an irritant, there was introduced a new phase, that is to say, the arterial vessels, as the animal came under the influ-

ence of the narcotic, were reduced in calibre. The changes of the circulation in this case were first marked in the retardation of the blood through the veins, then the vein increased in diameter, and there were signs of regurgitation of its blood; these indications were followed by what may be called irregular movements in the capillaries, and by reduction of calibre of the arteries. It was observed, nevertheless, that the narrowing of the arterial vessels, though well marked, was never so extreme as to prevent motion of the blood in them; that is to say, the degree of arterial contraction was limited. I consider this to be due to the circumstance that the animal had always ceased to breathe, and the further absorption of the narcotic vapor had consequently also ceased, by the time that the action of the vapor upon the arterial vessels was developed.

"During the period when the size of the arterial vessel was reduced, the motion of the blood in the capillary vessels fed by the arterial supply was modified; the blood flowing through the capillary channels moved less steadily, and was forced, if I may so express the fact, in pushes, as if there were intervals of relaxation of the arterial vessels during which the resistance to the impelling power of the heart slightly and slowly yielded. After a time the circulation of the blood through the artery became slower, the capillaries were left empty, the venous current ceased, and the condition of temporary suspension of all circulation, except slowly, in the arterial supervened. The effects here named were well marked from the action of the chlorides; they were seen under the influence of bichloride of methylene, they were still more definite under chloroform.

"The position then assumed, that the primary arrest of the column of blood during fatal narcotism is in the lesser circulation, we have to ask whether the arrest commences in the heart or in the lungs. The commonly accepted view has been that it commences in failure of the right side of the heart; but I incline to think that this view is incorrect, and that the positive source of failure is in the peripheral circulation of the lung. The vapor inhaled impresses, I think, *immediately* the minute circulation, and acts not by absorption into the blood, but by simple and instant contact with the minute pulmonary vessels, so that there is immediate resistance to the passage of blood through them. Three well-observed facts support this opinion:—1st, the fact already dwelt upon, that in cases of rapid death the lungs are emptied of blood; 2nd, that the arrest of the systemic circulation commences on the venous side of the circulation, and is attended with filling of the veins; 3d, that immediately after the death of the animal, if the chest be opened and the heart exposed, the right side of the heart, relieved of pressure, will immediately recommence to contract vigorously, showing that it is not itself paralyzed, but is restrained from action by mechanical resistance to its column of blood.

"If the theory of the action of narcotic vapors thus propounded be correct, we ought to draw from it this practical lesson, that in introducing new narcotic vapors into practice, the utmost care should be taken to select those only that are negative in respect to their action upon the vessels of the minute circulation. A gas or vapor that asphyxiates but does not irritate may be safer than a gas or vapor that does not asphyxiate and does irritate; for the former, when it kills, kills by a secondary process that is preceded by a series of symptoms foretelling the danger; while the latter, when it kills, kills often by instantly shutting off the column of blood that is making its way to the air, and by so oppressing the heart that every attempt at action, under the condition produced, increases the injury."

I would gladly add here but a single word as to the fatality of bichloride of methylene, now, according to Dr. Richardson's computation, as he himself told me, already administered some twenty to thirty thousand times. Had I most carefully sifted the hear-say reports which came to my ears while in London, I should have said there had been several, perhaps some eight or ten, deaths from this anæsthetic. Dr. Richardson, however, assured me there had been but *four* cases where methylene could be accused as the fatal cause, and these were not all, as he explained, quite satisfactory in proof. I cannot, however, from what I saw and heard, honestly turn from the use of ether to methylene, at any rate at present. It is to be earnestly hoped that such untiring study and investigations as Dr. Richardson's will not go unrewarded in the search for a *practical anæsthetic* free of the danger of chloroform and methylene and the disagreeabilities of these and of ether.

For years past, I have repeatedly heard many doubts expressed, even by medical men, except, of course, from those who had studied abroad, as to my statements in reference to the lack of acquaintance with

ether on the part of European surgeons. Since my return this time, many persons, both in and out of the profession, have spoken with me on the subject, and still with such a tone of incredulity as to induce me to present these facts and explanations here, and describe what I saw and heard and have done. To the majority of people, it is incomprehensible that ether should not be used and understood in England and on the Continent as it is here, in New England especially. Their doubts would be fully satisfied had they seen, as I did, the astonishment and, I will add, acknowledged satisfaction of the medical gentlemen who witnessed my administration of this anæsthetic in London.

But there is a monitor stands at my elbow in the person of my friend the Autocrat's iconoclast, who, in a stage whisper, says, "Doctor, *cui bono?* Do you really believe you have accomplished anything? Will they at all use ether now in England in preference to chloroform?" This is a fair question, and I will answer it truthfully by saying I really cannot tell, time alone will decide. I may, at least, say that the title heading these brief and desultory remarks, "Re-introduction of Ether into England," was the expression used in reference to my efforts by one of the most distinguished medical men in London. If, also, *causa humanitatis*, medical men going abroad to study, would follow up what I have commenced, I believe chloroform, as an anæsthetic where ether could have been used, will be more dangerous to the physician than the patient.

At the commencement of the International Ophthalmological Congress, August 1st, 1872, I read the following on the value of ether in ophthalmic surgery:—

Every week gives us substantial proof of the fatality of chloroform in operative surgery. A certain degree of anxiety, therefore, must always be present in the operator's mind, from which he cannot free himself, even if an equally competent person administers the anæsthetic. If these propositions are not true, then there is no force in what I have here to say. I do not advocate the use of ether because I come from the city where its employment in surgery was discovered and promulgated, but because I believe that there are others like myself who do not desire to run the risk of killing a patient with chloroform, and who, perhaps, would gladly avail themselves of ether were they rendered as familiar with its administration and harmlessness as we are in America. Let me be clearly understood. I use and advocate ether because it is as effectual as chloroform, and not dangerous to life. I should not hesitate to use chloroform, though perhaps not so freely as ether, did the latter not exist. As we have ether, I do not feel justified in using chloroform, and never have done so. I should not unless forced to; therefore it is that I desire to induce others to familiarize themselves with the use of ether. I believe that those who now use chloroform exclusively, and who can never do it without danger to the patient, would employ ether did they understand how to do so. I would unhesitatingly say that I think this is largely the reason why ether is not made use of; and I opine there is some indefinable dread of it, caused by the fatality of chloroform, simply because ether is an anæsthetic. Now I desire to here say at once that I believe it is difficult to kill any one with ether, and that death never occurs accidentally while it is being inhaled. The accumulated evidence on this point is sufficient for me at least, as the accumulated evidence of the fatality of chloroform is sufficient to deter me from ever using it unless forced by necessity. That ether is more difficult to take and to administer I recognize and appreciate as well as any one. I do not wish to be harsh with my medical brethren either at home or abroad, but I cannot but think that the disagreeabilities of ether induce some to run the risk of the use of chloroform, which may be fatal—when, who can foretell? I do not propose to enter into any discussion as to the action of ether, or the special methods of using it. I would, however, here say, that many of the diseagreeabilities of ether—nausea, vomiting, and headache—may be avoided or mitigated by the patients taking no, or but a light, supper the evening previous to the operation, and absolutely *no food whatever* the morning of the operation, which should, if possible, be done not later than 9 or 10 A.M. Ether, at the worst, is but a profound intoxication, and not unlike a drunken fit. On the other hand, thousands inhale it without trouble, as proved by their anxiety to take it a second time if necessary. A towel rolled into a cone, with a napkin or sponge pushed to the top of the inside, is all we need to pour our ether on, whilst our fingers can mould it over any mouth and nose. Some years ago I often heard in Europe medical gentlemen say, "But there are so many people who cannot take ether." I have yet to see one. The truth is, I believe, that surgeons who use chloroform are afraid of ether, and do not

dare to give enough of it at once in the commencement. Now if the patient is warned that the ether will choke him, and told when this occurs to take long breaths to relieve it, and not to struggle and endeavor to push away the sponge, many will go to sleep quietly and without trouble to themselves or the surgeon. I have but one other point to speak of in reference to giving ether. When the patient, whether old or young, struggles, and asks for a respite and fresh air, do not yield. Hold them down by main force if necessary, and at any rate keep the sponge tight over the mouth and nose till they finally take long breaths and then soon go off into ether sleep. Doing this prevents their remembering anything about their struggles. It is absurd to stop the ether and try to reason with adults excited by the anæsthetic, and cruel not to push on quickly with children. This may sound almost puerile to my American brethren, but my personal experience tells me that those who use chloroform have somehow a sort of dread of ether, as if it was to be suddenly fatal, and hence fail to give a patient enough to intoxicate him quickly. This arises from lack of familiarity with its use and administration.

In ophthalmic surgery there are several special reasons for the employment of ether. In the first place, death during any other surgical operation might be allowed to pass without creating undue or severe comment, but no community would let pass without it a death occurring from any anæsthetic during the extraction of a cataract, an iridectomy, or the removal of the globe. A possible fatal result is not included in ophthalmic surgery. In my own community I should not care to have a patient die from chloroform under my hands, and be myself tried for manslaughter afterwards. The prosecuting attorney could put scores of surgeons on the stand, whose evidence to the jury would be unanimous that I might have employed ether, which is not fatal, and hence the responsibility of the fatality of chloroform rested entirely upon me. It would be an ugly case. Another argument for ether is that it leaves the mind of the ophthalmic surgeon entirely free from anxiety or thought of the anæsthetic. When the operation does not require him to have an assistant, he may dispense with one to administer the ether, and give it himself, as a little care will enable him to avoid benumbing his fingers with the cold, and any bystander can hold the sponge over the patient's mouth while he sleeps. I do not mean that an assistant is not a convenience, but that with ether we may dismiss thought for the patient's pulse, &c., since blueness of the face over which we are working tells us when to take off the sponge and when to re-apply it. Moreover, operations are very frequent in ophthalmic practice, and an assistant to whom we may commit the administration of chloroform is not always at hand or readily obtained, whilst the necessity for immediate operation may from various causes be very pressing. For myself, I find there are but few ophthalmic operations which I hesitate to undertake alone under ether, when compelled so to do. I do not find that medical gentlemen feel thus about chloroform. Here let me answer by *experience* the *theory* that ether will not act favorably in many ophthalmic operations, in consequence of the nausea and vomiting liable to follow its use. In the ophthalmic hospital with which I am connected, some fifty Graefe's extractions are annually done, almost always under ether, without the subsequent nausea or vomiting, provided this takes place at all, seeming to interfere with the usual course of the recovery or the final result. Some three hundred other operations are also performed under ether. We never use chloroform. I will say nothing here of the use of ether in the general hospitals of my own city, as I desire to confine my remarks to ophthalmic surgery, in which I consider ether a blessing to both surgeon and patient. Finally, I would sum up what I mean, and would here say that ether is never fatal in surgery, it can always be used in ophthalmic practice, as it does not interfere with the operation or its results, it allows the operator to work alone if compelled to, and free from thought of the patient's condition. I admit that it is not so pleasant to give or take as chloroform. With those who can administer this latter without anxiety, and can rest at peace with their own conscience and the community in which they live after a fatal case under their hands, my remarks can of course have no force, and for such they are not intended.

Mr. Brudenell Carter said: "I much regret, Sir, the absence from among us of a gentleman who, more than any other in London, is in the habit of administering ether—Mr. Warrington Hayward, the surgical registrar and chloroformist to St. George's Hospital. He has advocated very strongly the use of ether in general surgery, but his experience is, and I must say that mine entirely coincides with it, that ether as an anæsthetic agent does not produce sufficient muscular relaxation to fulfil

all the requirements of the ophthalmic operator. As we have had it administered at St. George's Hospital, we have certainly found that the recti muscles have not been rendered passive in the degree that I should desire, and after some experience both Mr. Hayward and myself have determined to lay it aside, and return to our old and trusted friend chloroform, of which I must say we have no fear, and which we have never had any reason to regret using. It is with great deference that I venture to question the statement of Dr. Joy Jeffries about the safety of ether, but unless my memory plays me altogether false, there have been deaths from ether recorded in surgical history. I think that when anæsthetic agents were first introduced, and ether was the only one considered to be of any practical value, certain deaths did occur. I shall be grateful, Sir, if Dr. Jeffries will come to St. George's Hospital and administer ether for us, that we may see whether our past dissatisfaction with it may be in any way due to our faults of administration."

Dr. O'Leary observed that many differences in the effect of ether, when administered as an anæsthetic, were due to differences in the purity of the drug. He believed it to be a perfectly safe remedy, provided it be pure.

Mr. Jabez Hogg said that after much experience with various anæsthetics he preferred to operate without any anæsthetic. The resistance of the recti muscles was the best possible aid to extraction.

I replied, in answer to Mr. Carter, that a Medical Society in Boston appointed a committee some years ago to investigate all the reported cases of deaths by ether. That was done, and they could not find that any one of those reported deaths was due to the anæsthetic. When the muscles are too tense for the operation, I doubt if ether enough has been given.

Boldo.—This is the name of a new remedy which has been recently introduced into Europe. It is imported from Chili, where it is distilled from the leaves of a tree, of the genus *Monimiaceæ*. Its reputation appears to rest upon a pretty slender basis, and not upon the results of any trustworthy experiments. Thus far, it has been administered empirically for the more frequent affections of the liver. As in the case of *cundurango*, its use is most strongly recommended by charlatans, pecuniarily interested in its success, and like that drug, its popularity will probably be of very short duration.

Progress in Medicine.

REPORT ON OTOLOGY.

By J. Orne Green, M.D.

An Investigation concerning the Mechanism of the Ossicles of Hearing and the Membrane of the Round Window.—Burnett. (*Archives of Ophthalmology and Otology* vol ii., No. 2.)

Burnett has still further confirmed the theories of Ed. Weber and Helmholtz in regard to the mechanism of the ossicles of hearing, by some very exact experiments to determine the condition of the membrane of the round window during the conducting of waves of sound through these bones, and also to determine the effect of altered labyrinthine pressure upon the chain of bones and upon the membrane.

The preparations were made by removing the floor of the tympanum, leaving the membrana tympani, the ossicles and the labyrinth uninjured, and then placing them so that the movements of grains of starch, with which the parts were sprinkled, could be accurately examined and measured with a microscope.

The vibrations of different notes were then conducted against the membrana tympani, and the vibrations of the different ossicles and of the membrane of the round window observed. When the sound waves struck directly against the membrana tympani and the hammer, the ossicles and labyrinth responded to the vibration; but when the waves first struck the wall of the meatus, so as to be deflected, they were found to be destroyed, which seems to show that the bony walls of the auditory canal have no effect in conducting sound to the labyrinth. The excursions of the ossicles and of the membrane of the round window always bore a fixed relation to each other, and also to the pitch of the note, the longer excursion corresponding to the deeper note.

The experiments on labyrinthine pressure were made by opening one of the semicircular canals and attaching to this opening a column of water, the height of which could be varied, and it was found that when the pressure was increased or diminished beyond a certain point the excursions both of the ossicles and of the membrane of the round window ceased; sooner, however, during the occurrence of a high than of a low note. These experiments demonstrated a condition not unfrequently found in

the human ear, in which low notes are heard distinctly, but higher ones not at all. Such cases have been considered by some authors to be affections of those ultimate fibres of the auditory nerve corresponding to the unheard note, but these results of Burnett's prove that they may be merely faulty conduction from abnormal labyrinthine pressure. Politzer has already demonstrated, by a series of experiments somewhat similar to those of Burnett (*Archiv. der Ohrenheilkunde*, vol. vi., p. 1), that any object attached to the ossicula diminishes the extent of their vibrations, but that this diminution is greater with low than with high notes, and, as he says, these results agree with what is seen in practice, namely, that pathological products on the ossicula, such as adhesions, anchyloses or clumps of mucus, allow the higher note to be better heard than the lower, a result the opposite of intra-labyrinthine pressure, as demonstrated by Burnett. Such experiments as these, to be confirmed and extended, are thus gradually evolving a means of diagnosis in some of the more obscure diseases of the ear.

The following deductions are drawn by Burnett from his experiments.

(1.) The excursions of the chain of ossicles of hearing bear a fixed relation to each other.

(2.) The excursions of the ossicles of hearing are communicated through the labyrinthine fluid to the membrane of the round window.

(3.) The excursion of the membrane of the round window generally equals that of the stapes, but it may equal that of the membrana tympani at the point of the manubrium mallei.

(4.) The pressure within the labyrinth, increased beyond certain limits, causes cessation of the action of the membrane of the round window and the chain of ossicles of hearing.

(5.) If the labyrinthine pressure is greatly diminished or totally removed, the chain of ossicles may continue to vibrate, but they exert no influence upon the membrane of the round window.

Further Experience in Paracentesis of the Membrana Tympani, with Remarks.—SCHWARTZE. *(Archiv. fur Ohrenheilkunde*, vol. vi., p. 3)

Paracentesis of the Tympanum.—GRUBER. *(Allg. Wiener Med. Zeitung.)*

In the second volume of the *Archiv. fur Ohrenheilkunde*, 1866, Schwartze published an article on paracentesis of the tympanum, in which he favored the operation in a large variety of cases, not only of acute but of chronic catarrh of the tympanum. A short time after, a brochure on the same subject appeared and had an extended circulation. These two works he now supplements by a third, giving the results of his experience after a number of years, "the intervening time appearing sufficient for us to be able to judge of the final result of the operation in a large number of cases," and in the table which he gives at the end of his paper only those cases are quoted which he has been able to examine several years after the operation. The object of the operation is the removal of the secretion which has formed within the cavity as the result of the inflammation of the mucous membrane, and which impedes the free vibration of the membranes and ossicula. The present article is a review of the opinions expressed in the two previous works, and it appears that an extended experience has changed these views but little.

The convex, sack-like projections of the membrana tympani, and the yellow, triangular reflex behind that membrane, which were described as the only appearances seen on examination when there was an abundant secretion in the tympanum, Schwartze now modifies, as subsequent observation has shown that the membrane is frequently drawn inward by a collection of fluid in the cavity, instead of being forced outward, especially when this fluid is very adhesive.

This latter or collapsed condition of the membrane is very frequent, and the experience of all would probably agree with that of Schwartze, that it is the most common appearance of the membrane when adhesive mucus is present.

Small atrophied spots of the membrana tympani may be forced outwards by air driven into the tube, and resemble the convex projection of the membrane from collections behind it; attention once called to the possibility of their occurrence would prevent their being mistaken for fluid by any one of experience.

Another condition liable to deceive is the circumscribed swelling which is sometimes found on membranes subacutely inflamed; the differential diagnosis is here very easy, for, with such inflamed membranes, the cutis is so swollen as to conceal the manubrium of the hammer. Schwartze says he found this circumscribed swelling especially in commencing tuberculosis; it is unaccompanied by pain and the only subjective symptoms are a feeling that the ear is stopped up and occasionally a pulsation in the ear; the injection and swelling of the cutis and mem-

brana tympani are very marked, and the latter is of a yellowish red color; after several weeks, perforation of the membrane takes place, and its whole surface begins to suppurate. From the description given, it is impossible to think that Schwartze has anything more than one of the painless inflammations of the middle ear, which are seen in some severe general diseases, such as scarlet fever, and which might occur, although we have never seen it, without the general disease, especially in weak constitutions. Having observed exactly these same appearances in a severe case of scarlet fever, which resulted in the loss of both drum membranes, we cannot think that our author is justified in suggesting, as he does, a primary tuberculosis of the membrane.

The convex projections of the membrana tympani from fluid in the cavity, are *usually* associated with an enlargement of the radial bloodvessels of the cutis, and in some cases this congestion extends over the whole membrane, a condition described by older writers as pannus.

The most common appearance of the membrane, an abnormal concavity and a yellow reflex, with a line of demarkation and occasionally bubbles, Schwartze describes as other writers have done.

Attention is then called to a possible error in diagnosis, from an atrophied membrane being so collapsed against the bone that the yellowish reflex from the promontory resembles the reflex from a collection of fluid. In these cases, however, the reflex is seen in the centre and not at the lower part of the membrane, and the air-douche by separating the membrane from the bone removes the doubtful appearance.

Some cases in which fluid is present recover without an operation, from an improvement in the general health. The favorite German salt-baths Schwartze has used with benefit. If in chronic cases the improvement of the hearing does not last more than two or three days and then is lost completely again, he considers the paracentesis necessary, having found that in such cases a tedious treatment by other means produced but a temporary relief, while the operation, by thoroughly freeing the tympanum, gave a permanent cure.

In acute cases, the absolute necessity of the operation on the occurrence of dizziness, headache and vomiting is recognized, in order to diminish the danger of an extension of the inflammation to the brain. In scarlet fever and typhus the operation is urgently advised in the *beginning* of the inflammation, and likewise in certain cases of acute inflammation of the membrana tympani alone, as preferable to the superficial scarification recommended by Bonnafont and Gruber.

In regard to the character of the fluid, he had previously considered it serous in acute and muco-purulent in chronic tympanal catarrh, but a greater experience has shown him that the reverse may be and often is the case. In the majority of chronic cases he has found it purely mucus, very adhesive and transparent. Microscopically the secretion consists of mucus corpuscles in all stages of degeneration, with degenerated pavement epithelium, some fat and cholesterin. In 97 cases the secretion evacuated was serous 8, muco-serous 14, purely serous 67, muco-purulent 8 times. In bi-carbonate of soda the secretion remains unaltered for days, but in a 3-4 per cent. solution of caustic potash is rapidly dissolved.

For the operation, he still uses the lance-shaped needle recommended in his first work, preferring it to the modified Desmarres needle used by Troeltsch or the bayonet-shaped one of Lucæ. A cross cut he considers necessary only where the membrane is much thickened or where the secretion is very adhesive; suction by means of a Pravaz' syringe is not generally possible.

The accidents during the operation are unimportant and are enumerated as follows: vomiting, fainting, bleeding, injury of the wall of the labyrinth, difficulty of thoroughly clearing the tympanum on account of the adhesiveness of the secretion.

After the incision has been made the air-douche should be used, and this is sufficient, if the secretion is serous, to thoroughly evacuate it; if, however, it is thicker, suction by means of a Siegle's speculum can be tried, but it has generally been found necessary to force a stream of water through the whole ear, and thus wash it out. If both ears have been operated on, he injects lukewarm salt water rather forcibly into the nose, and a part of it runs out through the ears; if only one ear has been operated on, he accomplishes the same purpose by injecting through a catheter directly into the Eustachian tube. The method of washing out the tympanum proposed by Hinton, which we have used with most satisfactory results and prefer to all others, as more simple and less disagreeable to the patient, is not mentioned at all by Schwartze. It consists in inserting the nozzle of the syringe, air tight, into the meatus by means of a rubber cork, then slowly forcing the water through the Eustachian tube into the nose.

Where the membrana tympani is thin, the pain from the operation is often wanting; where the membrane is thick it may last for some hours. After the operation the ear should be closed, the patient kept quiet on restricted diet, and everything tending to congestion of the head and ear should be avoided. The air-douche should be repeated on the first and second day after the operation; usually, by the third day the incision is healed, but it may remain open much longer.

Inflammatory reaction after the operation, is rare, but in Schwartze's experience is more common than Troeltsch and Politzer have said: he has found it to a greater or less degree in 20 per cent. of all his cases, while Politzer has never seen it. It may be confined to the membrana tympani or may involve the meatus, auricle, and sometimes the whole tympanum. Polypoid granulations on the edges of the wound or the wall of the meatus may form and require removal and cauterization. The inflammation usually begins on the third day after the operation.

The second day after the operation, the ear should be manipulated as little as possible, but if, after the incision has healed, moist râles are still heard by auscultation, alkaline solutions should be injected to render the mucus more liquid in order that it may be absorbed, and the râles should disappear before treatment is given up. To prevent relapse, care should be taken that the Eustachian tube is permeable, and, if necessary, the naso-pharyngeal cavity and the tube should be treated by injections, and the general health attended to.

The greater the improvement after the air-douche, the better will be the result of the operation: if, however, there is no marked improvement, or the symptoms render a disease of the labyrinth probable, a perfect result can hardly be expected. Disease of the membrana tympani itself does not, however, necessarily, exclude a perfect result, as Schwartze has obtained such where the membrane was atrophied, and also in cases where it was thickened and at the same time relaxed: if, however, it is found thickened, tensely stretched and much drawn in, the improvement from the evacuation of the secretion is generally but slight. In chronic cases, the evacuation of the secretion is generally followed by immediate improvement in the hearing, but in acute cases we may have the opposite, the hearing being worse for a short time. Schwartze has found that these latter cases are apt to be of long duration.

Finally, the results of the operation in 100 individuals and 163 membranes are given, the majority of the patients having been seen and examined from four to six years after the operation. Of these 163 operations, 87 resulted in perfect cure, 56 were much improved, 15 slightly improved, and 5 not benefited at all. Of the 100 patients, 81 were under 20 years of age, and the duration of the disease, in all but 34, was more than a year. The duration of the treatment varied between eight days and four weeks. The operation required to be repeated in 43 cases.

Schwartze claims that the operation not only shortens the duration of the treatment, but that relapses are much less frequent than under the old treatment by means of the catheter and internal medication. In individuals predisposed to catarrhal diseases, relapses do, however, occur, and in some cases a permanent cure is impossible, especially in cachectic patients and in those with ulcerative rhinitis; these are, however, exceptional cases.

Thickening of the mucous membrane and abnormal adhesions in the conducting apparatus are much less common with the mucous secretion than with the serous exudation.

The article closes with a quotation from Velpeau (1839): "La perforation du tympan en est encore à prendre rang parmi les operations utiles et réglées de l'art de guérir."

Gruber agrees fully with Schwartze in the advisability of the operation for the removal of mucus or pus from the tympanum, both in chronic and acute cases, and in the case of purulent or hæmorrhagic exudations considers it as a true *indicatio vitalis* to diminish the risks of the inflammation extending to vital parts. In simple catarrh he recognizes the possibility of the absorption of even large collections, and would, therefore, not hasten the operation till other means of getting rid of the secretion had failed or threatening symptoms appeared.

He mentions, however, another and new indication for the operation in those cases in which a perforation has taken place on the anterior and lower quadrant of the membrane insufficient to thoroughly evacuate the tympanum, either from its small size or from the adhesiveness of the exudation. In these cases, the retained secretion is a direct irritation which not only prolongs the disease but is also liable to cause severe pain from the pressure on the inflamed membrana tympani. For the re-

lief of this condition, Gruber proposes two courses, either to enlarge the natural perforation or to make a new opening in the posterior segment of the membrane at a considerable distance from the first perforation, and in performing the operation care should be taken to make the opening sufficiently large, and to leave as much membrane as possible between the natural and artificial openings, as experience has shown that the smaller this bridge of tissue the more difficult it is to obtain cicatrization.

Cases of inflammation and thrombosis of the transverse and petrosal sinuses with otitis media purulenta.—Schwartze [*Archiv f. Ohrenheilkunde.* Vol. vi. p. 3].

The diagnosis of thrombosis and phlebitis of the sinus transversalis, as the result of purulent otitis media, can only be made with certainty after pyæmic symptoms have set in. An unequal filling of the jugular veins, which was considered by Gerhard pathognomonic of the disease, is not considered of value by Schwartze, as in a case examined by him, in which the right jugular vein was enormously swollen, a large thrombus was found filling the right transverse and petrosal sinuses completely, while the sinuses of the opposite side were entirely free, a condition the opposite of what Gerhard would have expected. A painful œdema over the mastoid process, which Griesinger considered pathognomonic, is found so often, not only in caries of the bone, but also in all kinds of inflammation of the middle and outer ear, that this symptom cannot be considered of value.

Schwartze then gives the history of four cases, all of them associated with purulent inflammation of the tympanum, and in three of them he considers the inflammation of the sinus was the result of the otorrhœa: in only one of them was the disease of the sinus diagnosticated during life from the existence of pyæmic symptoms; in the others the symptoms were those of meningitis, the patients dying before the pyæmic symptoms appeared. In the first case, there was no caries of the bone, the inflammation apparently extending along the small veins of the bone from the mastoid cells to the sinus; in the other cases there was caries of some part of the tympanum.

In addition to the four cases by Schwartze, Dr. Stokes narrates a case in which there was likewise no caries of the bone. The symptoms were chiefly delirium and slight swelling behind the ear, which led him to perforate the mastoid cells, but no pus was evacuated. Death followed on the thirtieth day. In De Rossi's case there were pyæmic chills, with death in three weeks. The transverse sinus and upper end of the jugular vein were filled with purulent thrombi; there was also here no caries of the bone.

In a series of dissections previously reported by Wendt (Archiv für Heilkunde, xi.), he examined thirteen cases of disease of the brain associated with disease of the ear, and in six of them found a thrombus in the lateral sinus and in one a thrombus in the cavernous sinus. In one of these seven cases of thrombosis the disease of the sinus was caused by a tumor of the neck and was not connected with the ear-disease, in four of the cases the connection with the ear could, however, be distinctly traced, while in the other two cases it was highly probable, although not positively proven. Caries of the petrous bone existed in only five of the cases.

Remarks on Affections of Hearing in cases of Disease of the Nervous System. Hughlings-Jackson. [*Medical Times and Gazette*, July 13, 1872.]

Dr. Hughlings-Jackson, in a clinical lecture on deafness in nervous diseases, after calling attention to our imperfect knowledge of this field, remarks that in physicians' practice an excellent opportunity is afforded for the precise study of the various causes of such deafness by more accurately establishing the nature of the deafness during life and noting the appearances in the ear and its nerves post-mortem.

Dr. Jackson states that, excluding the cases of disease in the outer and middle ear, deafness is a rare complication of intra-cranial disease, much less common than optic neuritis. In no case has he ever seen deafness the result of adventitious products or of any disease of the cerebral hemispheres, meaning, apparently, when the disease was confined to the hemispheres, as immediately after he gives two cases in which deafness was a prominent symptom, one where a tumor was situated at the junction of the pons and medulla oblongata and another where a tumor extended from the sphenoidal fossa back to the auditory nerve. Thus deafness becomes in these cases a symptom by which the position of the disease may be located, while optic neuritis is of no value, as it may occur from a tumor, probably in any part of the encephalon, the adventitious product scarcely ever involving any part of the optic nervous system.

In both of the cases quoted above, the deafness was double, while but one nerve was involved, and Dr. Jackson is unable to give any explanation, as he has seen a tu-

mor of the cerebellum producing deafness only on one side.

An auditory neuritis is as yet undetermined, but would probably depend on disease of the cerebellum; but Vulpian states that trouble with the hearing is very rare in alterations of the cerebellum.

Dr. Jackson then calls attention to the association of epilepsy with disease of the tympanum and, although considering the connection between the two as uncertain, says that it is as important to inquire for this as for any other excentric causes. The irritability of nerve-tissue in epilepsy he thinks depends on different pathological processes, among others embolism and thrombosis, and hence in an epileptic patient he inquires for heart disease, rheumatism, scarlet fever, and also for discharge from the ear, as this may produce venous thrombosis; the epileptiform seizure, he thinks, may depend not on reflex action from the ear disease but from the gross process of thrombosis. These facts, however, have never been confirmed by autopsy.

[To be concluded.]

Reports of Medical Societies.

NORFOLK DISTRICT MEDICAL SOCIETY.

The Society held its *quarterly meeting* at Hyde Park, on Wednesday, July 10th, the President, Dr. Jarvis, in the chair. The subject announced for consideration was "Medication in Disease."

Dr. Maynard, of Dedham, in opening the discussion, alluded to the common tendency to hasty generalization, premature conclusions being too often drawn from limited facts. This propensity to promulgate hypotheses, or theories constructed upon insufficient data, was, in fact, one of the greatest hindrances to the progress of true medical science.

He next illustrated the importance of obtaining a true knowledge of the natural course of disease uninterfered with by medication, affirming, in conclusion, that medication in disease is likely to be of value in proportion to our knowledge of disease without medication.

Dr. Maynard's paper appeared in full in the Journal for August 29th ult.

Dr. Monroe, of Medway, spoke of the very great advance that has of late years been made with regard to our knowledge of therapeutics, and showed how this increase of knowledge, in revealing the existence of former errors, had thrown a certain amount of discredit upon the regular profession, which has been augmented by a very extensive misapplication of drugs. Considering that too great doubt already prevailed with the public as to the utility of medication, he deemed it unwise, that any utterances of medical men should add to the prevailing scepticism. The medical journals of the day unite their testimony in favor of numerous drugs, while the question of the power of other agents is placed beyond a doubt by the physiological experiments of the most capable and trustworthy observers. He adhered to the opinion that certain diseases may be cut short by the administration of drugs. Diseases, for instance, of an inflammatory type may be shortened in duration by the administration of cathartics and emetics, as formerly employed. The fashion now prevailed, however, of avoiding, as far as possible, all active interference in disease. He was inclined to the belief that we had already gone too far in the direction of non-medication, and it was becoming more and more evident that public opinion among the profession, if not among the laity, was slowly reverting to former belief, and more in favor of active interference. It was this feeling of scepticism among the profession that had thrown so many patients into the hands of homœopaths, spiritualists and other uneducated practitioners. When a man has some trifling indisposition and seeks relief from his medical adviser, he is very apt to be put off with some indefinite, general directions as to rest and diet. With this advice, however, he is often far from satisfied. It is possible that his business affairs may demand his immediate attention, and he is convinced that, at least, an attempt might be made by the aid of some drug to alleviate his symptoms. Hence, it happens that in his anxiety to obtain relief, he too often falls into the clutches of the charlatan, fertile in expedients and dazzling in promises, and, although he derives no benefit, even from this source, he consoles himself with the idea that every effort has at least been put forth in his behalf.

Dr. Morison, of Quincy, referred to the unfortunate tendency of passing from one extreme to another in the practice of medicine, regardless of the truth expressed by the proverb, "*in medio tutissimus ibis.*" While our fathers administered their potions with no sparing hand, there existed at the present day a latent disposition to ignore

the efficacy of medicines in the treatment of disease, and to trust chiefly to the conservative forces, or to the recuperative powers of nature. He spoke of the different means at our disposal for allaying speedily nearly all forms of pain, and thought, that if no other aid than this could be afforded by the physician, his office would be a noble one. We could remove affections dependent on parasites. He believed, moreover, that other morbid phenomena could be directly controlled by the proper use of drugs, although it was not always easy to explain the *modus operandi*.

Dr. Salisbury, of Brookline, said that with regard to certain diseases of the inflammatory type, such as pneumonia, their natural course had already been studied, and was perfectly understood, and experience had shown that when such cases were properly treated the amount of suffering was lessened, and the duration of the illness shortened, and he therefore failed to see how a physician, in view of these facts, could conscientiously leave a patient without medication.

Dr. Tucker, of Stoughton, remarked that an extensive experience of above forty years, had tended to strengthen his confidence in the power of most drugs; at the same time he was conscious that there might arise an over-confidence in the power of medicine, which would very naturally result in a tendency to over-medication. In this way, he was willing to admit, evil had been done, more especially, perhaps, in the over dosing of young children. He enumeratcd finally the advantages to be derived from venesection, endorsing fully the views of Dr. H. I. Bowditch on this subject, as expressed in a paper read some time ago before the Massachuetts Medical Society.

Dr. Cotting, of Roxbury, expressed the conviction that the continued tendency of the time was to administer drugs where no beneficial effect could reasonably be expected from any medicinal agent. This was an evil greatly to be deprecated, the responsibility of which should be shared equally by the public and the profession. When a physician is summoned to the bedside, whatever may be the ailment, a counteracting medicine is confidently expected by the patient, and this idea is tacitly acquiesced in by the physician. The sufferer had perchance experienced, previously, the power of opium in allaying pain, and he is allowed to believe that analogous effects can be produced by other drugs in the way of breaking up and eliminating disease, just as intermittent fever is supposed to be broken up by quinine. A great good would be accomplished, if this idea could be eradicated from the minds of the profession as well as of the public, for no greater obstacle existed to real advance in the science of therapeutics. The idea, then, should be inculcated that the province of the physician in the sick chamber is not to open an immediate attack upon some hypothetical *materies morbi*, but rather to put the patient in the best possible condition for enduring his disease, if it must run its course; or otherwise, by regulating his diet and surroundings, to protect him from the concomitant dangers of his illness, just as the pilot, in the descent of the St. Lawrence, keeps the craft clear from the sunken rocks; in fact, to give him the best treatment his disease and his situation admits of, without trying any hazardous experiment *(anceps remedium)* for the sake of seeming to do something.

It should be borne in mind that few, if any, of the vaunted specifics had stood the test of time. As to cinchona, or its active principle quinine, which had been referred to by previous speakers, it was not an unmixed good, even as commonly exhibited in intermittent fever by some experienced physicians. This disease had been successfully treated long before the discovery of cinchona, and even now recoveries were made without resort to this drug. It almost seemed to him, at times, as if it might have been better if cinchona were never known, so much false argument has been based upon its reputed effects in fever and ague, to the great injury of the sick in other affections. He remembered, moreover, to have heard it stated by an eminent physician in a malarious district that some of the most obstinate forms of disease which had come under his observation were cases of what he called *quininitis*, or that condition of nervous prostration and disorder induced by over-doses of quinine; and analogous results had been observed in other localities from the heroic doses of mercury and jalap administered in epidemics of yellow fever. Another source of error had its origin in comparisons drawn from the effects of animal and vegetable parasites upon the human body, and the occasional speedy removal of these effects by the destruction of the parasites. This subject, however, should be considered as pertaining to natural history rather than to pathology; and as to the disturbances produced by the presence of parasites, these should be likened rather to the results of injuries, such as perforations, abrasions or burns.

The effects of human parasites are very rarely fatal, nor do they of necessity produce any serious results. Although in individual cases for the most part readily destroyed, their continuance has been very carefully provided for in nature, and there was no probability that any single species would ever be entirely extinguished by human means. Dr. Cotting spoke of that form of delusion commonly known as homœopathy, the spread of which had been alluded to as surprising, and quoted the assertion of a well-known professor, made, by the way, before its appearance hereabouts, that one-fourth of the practice of any given locality would, ordinarily, gravitate toward irregular practitioners. He had previously shown that in this vicinity, in recent years, less than one-sixth of the practice had been in this way diverted. When homœopathy was first introduced into this region, Dr. C. (then a student) had compared results with a fellow-student inclined to that persuasion, and demonstrated, by actual trial, that just as favorable effects could be produced by the use of colored water as with the best imported German pellets, and in this way had the satisfaction of convincing his friend of his error. As to the prevalence of homœopathy, that had by no means attracted so many followers as did Thompsonism thirty years ago, when entire communities, especially in country places, appeared to be carried away by that delusion, so that educated physicians in such localities were, for the time being, almost driven from the field. The present popular delusion is now on the wane, its fundamental theories having been already abandoned by its former supporters, so that those who nominally practise in accordance with its tenets are found in point of fact to rely, in emergencies, upon ordinary drugs, given in effective doses. The coming practitioner of the regular school need not concern himself with regard to this form of quackery, which will soon disappear, but to arise, however, in some new guise.

It is important in speaking of diseases, not to be led astray by imperfect nomenclature. Some affections, such for instance as *cholera-morbus* (often confounded with sporadic cholera), should be regarded, not so much in the light of diseases, but rather as disorders :—efforts, perhaps, on the part of the stomach and bowels to dislodge ingesta too crude for assimilation, or, in a disordered state of the bowels, too burdensome to be retained. In such cases, emetics and cathartics might certainly be of use, to assist in expelling offending matters. Dr. C. insisted, however, in concluding, that the educated practitioner should strenuously resist that outside pressure, which demands under all circumstances a *recipe*. Nor did this idea involve any real distrust of the power of drugs. It required simply, that the physician should be allowed to decide whether, or not, the administration of a medicine was necessary; and should, moreover, be permitted to withhold all drugs, without danger of aspersion, in cases where, in his judgment, no actual service can be rendered by their use. It was to be feared that a popular failing in this respect had caused not a few to confound the meaning of the terms recovery and cure. At all events, a physician in attendance should have an undisturbed positive, not a negative direction of the case; should withhold or energetically put in force known resources of his science and art; should guide, and not be led.

Dr. Amory, of Brookline, thought it was a mistake to suppose that the peculiar effects of a drug could be exerted in disease, and not in health. This error arose from a general misapprehension, or ignorance, of the physiological action of drugs, that is, their effect upon the various organs of the body, and on their functions, as ascertained from experiments upon animals, as well as upon the human body. It was by this means that we had arrived at a satisfactory method of determining the indications for the use of bromide of potash in a certain class of lesions of the nervous system, connected probably with a determination of blood to the minute bloodvessels.

He considered it important to bear in mind that the term "medication" is not to be limited to the mere exhibition of drugs, but must include, also, whatever tends to produce any actual change in the condition of the patient, such, for instance, as the purifying of the atmosphere and regulating the temperature of the sick room, making external applications, &c. This fact is illustrated by the very decided effect sometimes produced by simply reducing the temperature of the body, a principle which has been successfully applied by Niemeyer to the treatment of scarlatina by the application of wet sheets. "Medication," then, should properly include all that is done to relieve the patient. He maintained that the physician should be cautious about prescribing drugs, the physiological effects of which are not clearly understood. An exact knowledge of the physiological action of each drug, combined with a familiarity with

the organic and functional disturbance accompanying each disease, will furnish the sound practitioner with the best guide to rational medication. It was a neglect of this principle that had led to so much empiricism. Dr. Amory criticized a recent assertion of Rev. Dr. Hedge, at the last annual dinner of the Society, to the effect that medical men were very largely indebted to empiricism, especially in the department of therapeutics. He quoted the definition of the word "empiricism" as given by Webster, showing that it implied an absolute ignorance of pathology, and demonstrated that our advance in the knowledge of disease and treatment, was the direct result of the observations and experiments of men like Bernard, and could not with propriety be ascribed to the result of empiricism in any form.

The President, Dr. Jarvis, in closing the debate, adverted to the wide difference of opinion prevailing among medical men as regards the therapeutic virtues of medicinal agents. The only rational course left, therefore, to the honest physician, was to "prove all things, and hold fast to that which is good." The whole past history of materia medica is replete with the story of the rise and fall of individual drugs. Each separate drug had its turn of being held in high esteem, and gradually falling into disuse. He expressed the hope and belief that with the advance now being made in our knowledge of materia medica, the time would soon come when the popularity of each drug would depend, not upon an indiscriminating, empirical belief in its curative effects, but would rest rather upon the firmer basis of scientific experiment and clinical observation.

Dr. S. E. Stone, of Walpole, read a paper on "Medication in Insanity," which appeared in this JOURNAL for Sept. 26th ult.

Dr. Campbell reported a fatal case of narcotism in a child fourteen days old, probably induced by a dose of chloral and morphia administered to the mother. This paper was published in this JOURNAL Aug. 15th ult.

Dr. Williams, of Roxbury, read a paper entitled, "Normal Distortion of the Pelvis," which appeared in this JOURNAL Sept. 12th ult.

The President gave notice, that the subject selected for discussion at the next quarterly meeting was "Lung Diseases in Infancy."

Medical and Surgical Journal.

BOSTON: THURSDAY, OCTOBER 3, 1872.

ETHER IN A NEW LIGHT IN ENGLAND.

FROM our recent correspondence with English medical men, we have learned pretty conclusively that anæsthesia by the inhalation of ether as practised by us in America is not fully understood by them, and that, to this cause, perhaps, more than to any other, is due the dissatisfaction felt by them while employing it; that their complaints of insufficient relaxation of the muscular system and but partial insensibility can be explained only by their want of knowledge; and we are convinced that, when these objections are removed, a reversal of opinion will take place and ether be more freely used across the water.

The subject has been so thoroughly discussed in America during the past twenty-five years, that no new ideas can be advanced; but, as an episode in the history of anæsthesia and an important one, as we believe, we call attention to the article of Dr. Jeffries in this number of the JOURNAL. In this he refers to the state of feeling existing in England regarding the use of ether, and relates his own experience in "re-introducing" etherization to the most eminent surgeons in London. If the course taken by Dr. Jeffries brings about the result which might naturally be expected, he certainly has conferred a boon on our English brethren and their patients.

Ever since the introduction of ether, this JOURNAL has maintained the statement, confirmed by the most eminent men of the profession, that, in the legitimate use of ether by inhalation, no bona fide case of death is on record or has ever taken place; that it is, therefore, whether given for a brief space of time or for days together, *absolutely safe*, while the deaths from chloroform are numbered by hundreds, the latter agent killing stout, healthy patients *without warning*—killing as lightning does, by a special shock peculiar to itself. The delay caused in

bringing the patient under ether is constantly advanced as an argument against its use; but every one knows that from three to six minutes is a fair time in which to attain full anæsthesia and relaxation of the muscles, and chloroform can do no better. The nausea, headache and vomiting charged to it are simply accidents which do not occur in *our* patients, simply because we take means to avoid them. The inability to obtain full insensibility is a myth of English surgeons, which Dr. Jeffries has taken means to dispel; and, beside these, the most ardent opponent of ether can advance but a few trifling objections to its use. To our mind, however, one argument alone should satisfy every conscientious man and should govern his action—*chloroform is dangerous, ether is safe.*

The whole difficulty experienced by the English is solved simply by saying that they have not, hitherto, understood how to use ether—or rather that they have constantly ascribed to ether the same dangerous effects they have so widely feared in chloroform, and have administered the former as they have been accustomed to give the latter. To give ether on a thin cloth, *guttatim*, with a free admixture of air, to remove the towel on the struggle which announces the first stage of anæsthesia, to manifest a dread of the stertorous breathing, and other signs of full etherization—this we know to be the custom of English surgeons; but their custom is not ours, and if Dr. Jeffries has impressed on their minds the true use of ether by his practical demonstrations, he has, without doubt, paved the way to its speedy use in England in place of chloroform.

Deaths from Chloroform.—In furtherance of our plan to note statistically the deaths from the use of chloroform recorded by our contemporaries, we give below a list of those noticed during the present year. In our previous lists we have reported twenty-nine cases.

30.—Reported by Dr. Ira Perry, Assistant Surgeon 9th U. S. C. T. *Circ. No.* 3, *S. G. O.*, 1871. Male, aged 20. Excision of portions of femur.

31.—*Brit. Med. Jour.*, Nov. 25, 1871. Male, aged 33. Amputation of toes.

32.—Dr. Pirrie, *Brit. Med. Jour.*, July 29, 1871. Male, aged 37. Operation for hernia.

33.—*Brit. Med. Jour.*, Dec. 2, 1871. Male, aged 53. Amputation of toe.

34.—Dr. Muscroft. *Cincinnati Lancet and Observer*, January, 1872. Male. Reduction of dislocation at elbow.

35.—J. Priestley Smith, Birmingham Eye Hospital, *Brit. Med. Jour.*, April 6, 1872. Male, aged 26. Operation for iridectomy.

36-7.—Dr. Von Köpl, *Sitsungs Bericht des Vereins der Aerzte in Steyermark*, 1871. Two cases.

38-9.—Dr. Körner, *ibidem*, two cases.

In-growing Toe-nail.—Dr. Stillwell, of Epsom *(British Medical Journal*, July 13th ult.), says that for the removal of this affection, his "invariable mode of proceeding has been to find the edge of the nail with a probe, and then to remove the granulations and hypertrophied cellular tissue, on both sides, if requisite. In no case have I been disappointed, or ever had to treat the patient for the return of this grievous complaint."

This mode of treatment, though differing in the principles on which it is founded, is not very unlike that for a long time resorted to by one of our neighbors, who has repeatedly reported his cases to various societies. An account of his operation may be found in the Journal for May 24th, 1866. In both these operations the necessity of "the un-scientific, cruel operation of tearing out the nail," as practised by Dupuytren, is unnecessary. A paper on the subject was promised us some time ago, and we hope soon to receive it. Though not a vital complaint, it has often proved exceedingly troublesome, to both operator and patient.

The Prussian Government publicly calls attention to the presence of trichinæ in hams imported via Bremen from the United States, warning purchasers to abstain from them, and threatening the sellers with criminal proceedings.

From Continental Journals.

HYPERTROPHY OF THE TONGUE.—Dr. G. Maas reports that five cases of hypertrophy of the tongue have occurred during the past year in the surgical clinique at Breslau. The enlargement was in each case congenital, sometimes involving the entire organ, at others being limited to a lateral half. The part affected was in each case removed by means of the galvano-caustic ligature. The microscopical examination of the removed part showed that in one instance (that of a child two months old), the enlargement was the result of simple hyperplasia of all the textures of the tongue. In three other cases there was found to be a new formation of connective tissue and bloodvessels, so that the tongue was enveloped in a spongy, cavernous mass. This formation of new texture had attained the greatest magnitude in the case of a patient twenty-one years old, and was the least marked in the case of a child three years old. The writer concludes, that hypertrophy of the tongue begins always with simple hyperplasia, to which is afterwards added, as a secondary lesion, an increased development of the connective tissue and bloodvessels, this abnormal growth being stimulated by the pressure the enlarged organ receives from the surrounding parts, the pressure being so great in some instances as to force it from the mouth.

[*Note.* Hypertrophy was the term formerly employed to indicate any abnormal enlargement of the body. The distinction indicated by the term *hyperplasia* was first made by Virchow, to represent any abnormal enlargement arising from an increase in the number of the original elements. "Hypertrophy" was then limited to that form of enlargement depending upon the increase in the size of the primary elements, such, for instance, as is seen in the pregnant uterus.]

ANCHYLOSIS OF THE LOWER JAW; RECOVERY THROUGH THE FORMATION OF A FALSE JOINT ON BOTH SIDES.—This case occurred in the same clinique as the preceding. The patient was twenty-seven years old, and was affected with complete anchylosis of the jaw, the result of an inflammation following scarlet fever. The growth of both upper and lower jaw was appreciably retarded, in consequence of continued disuse. Perfect relief was afforded by an operation, which consisted in removing a wedge-shaped portion of the lower jaw, the base of which pointed downwards. At the time of the report, four months after the operation, the motion of the jaw was completely restored. (*Arch. f. Klin. Chirurgie*, xiii. *B.* 3 *H.*)

THE RELATION OF THE CHORDA TYMPANI TO THE AUDITORY NERVES.—It is well known that when the electric current is applied to the tympanum of the ear, a peculiar sensation is experienced along the border of the tongue, and if the irritation is increased, it may be felt as far as the extremity of that organ. A similar effect may be produced by the application of very warm or very cold liquids to the tympanum, or by irritating a certain portion of that organ in any way. In other instances, the irritation of the tympanum produces a taste in the mouth like that of copper. M. Duchesne, and later M. Philipeaux, of Lyons, have called attention to this phenomenon, as being an important physiological sign, which would enable the aurist to determine in cases of deafness the degree of sensibility of the nerves of hearing. M. Bonnafort, at a meeting of the *Academie de Medicine*, held July 16th, 1872 (*Le Mouvement Medical*, Aug. 10, 1872), demonstrated that this phenomenon possesses really no clinical value whatever, and cannot, therefore, be employed, like the tuning fork or watch, in determining the degree of sensibility of the auditory nerves. This will be understood by referring to the anatomical relations of the several organs. He showed that the sensation experienced in the tongue is the result of the irritation applied to the chorda tympani, which is thence transmitted to the hypoglossus by means of the anastomosis which unites those two nerves, whereas, no communication has yet been discovered between the auditory nerves and the chorda tympani. The chorda tympani may be completely destroyed, and the tongue absolutely insensible to any application of the electric current, and yet the sensibility of the auditory nerves is not diminished, and, vice versâ, the auditory nerves may be completely paralyzed without interfering with the sensation in the tongue transmitted in the above manner from the tympanum. There is no reason to suppose, then, that the irritation of the chorda tympani can produce any appreciable effect upon the auditory nerves.

SEA BATHING.—Dr. F. W. Beneke, of Marburg, has contributed a series of articles to the *Berliner Klin. Wochenschrift*,

enumerating the advantages to be derived from a visit to some of the principal seashore resorts of England and the North sea. He gives his own experience at these places, and furnishes some useful hints upon the influence of sea air and sea bathing. These effects, it is shown, differ very essentially, according to the locality of the shore, and the season of the year. For patients suffering from general debility, whether inherited or acquired, he recommends the invigorating autumn air of southern England. The beneficial influence of this atmosphere shows its effects in most patients by the marked increase of appetite and bodily weight, and the enjoyment of a natural, refreshing sleep. These resorts are likewise recommended for scrofulous children; for those having a disposition to tubercular affections; for women reduced by repeated child-bearing and long continued nursing, as well as for those debilitated by overwork. The desired improvement must not be expected, however, within a fortnight or three weeks. The more deeply seated the constitutional affection, by so much longer must the sojourn at the seashore be extended, so that many should remain there months, where they now limit their stay to weeks. If sea bathing is also resorted to, it must be borne in mind that this is a much more powerful therapeutic agent, and should therefore be indulged in by invalids only under certain limitations, and with the sanction of a competent adviser. Many would derive greater benefit if they would content themselves with trusting to the sea air alone, without venturing into the water. Others neutralize the effect of sea air and bathing, by subjecting themselves to the additional fatigue of a long daily walk, and go away weaker, perhaps, than when they arrived. Where the invalid expects to add to his stock of physical strength, he cannot afford to make an injudicious use of the power which is still left him. Those alone can hope to derive benefit from the bath, whose nervous system is still strong enough to react after the vigorous shock which accompanies immersion. It is also important that the digestive apparatus should be in such a condition as to enable the patient to respond with safety to the increased demands of his appetite. Finally, those alone should indulge in the sea bath, whose sleep is rendered thereby deep and natural, and not made short and broken. The propriety of continuing the daily bath for any length of time in any individual case, depends upon the physical condition of the invalid, to be determined by a careful observation of the effect of the first one or two baths. Of course regard must be also paid to the temperature of the water, as well as that of the surrounding air, which may vary materially upon successive days. In cases where open sea bathing is for any reason not to be advised, a milder tonic effect of sea water upon the skin may still be derived from in-door baths, followed by friction with brush or towel.

It has been shown by physiological experiments, that the effect of the sea air, and in a less degree that of the sea bath, tends to accelerate the elimination of the nitrogenous elements of the body, while that of phosphoric acid is diminished.

Electricity a Means of Detecting Death.—Dr Rosenthal, of Vienna, reports a number of cases, and also a series of experiments upon animals, which illustrate the important aid afforded by electricity in furnishing a tolerably certain test for determining death. This sign is the contractility of the muscles in response to the application of the electric stimulus. By the aid of this test, it is affirmed that the diagnosis is rendered easy in cases of suspended animation, arising from apoplexy, suffocation, drowning, &c. It will be found also of practical utility, it is thought, upon the battle field, and in time of extensive epidemics, when the danger of infection necessitates the speedy burial of the dead. Among the cases quoted is that of a hysterical young woman, aged twenty-four, who had lain for thirty-two hours, evincing during this time most of the ordinary signs of death. Some suspicions having been aroused, however, as to her actual condition, to settle all doubts, it was decided to summon Dr. Rosenthal. He found, upon his arrival, that the face as well as the entire surface of the body, exhibited the marble pallor peculiar to the corpse, while the skin was everywhere cool to the touch. Upon raising the eyelids, both the pupils were found to be equally contracted, and showed no appreciable reaction to the influence of light. The upper and lower extremities were relaxed, and when raised, fell like any dead weight. No pulsation of the heart was perceptible to the touch, nor of the radial arteries at the wrist. Upon applying the stethoscope to the heart, however, the room being perfectly quiet, he was able to make out a suppressed, intermittent beat. The thorax, when uncovered, was found to be motionless, but upon carefully watching the abdomen, an exceedingly weak and slow

motion could be detected in the lateral walls. The ordinary respiratory murmur was not heard. Dr. Rosenthal now applied his proposed test, and found that the various muscles responded readily to a feeble excitation, so that he was able to give the assurance that the case was one of suspended animation. In accordance with his directions, therefore, heat was applied to the body, and vigorous friction maintained, the result of which was, that at the end of forty-eight hours the woman gradually recovered the power of speech and motion. She stated, that during the first part of her lethargy, she was perfectly unconscious, but afterwards overheard distinctly the remarks of those about her, with regard to her death, without the power, however, of being able to make any motion, or emit the least sound. (*Wiener Med. Presse*, May 9 & 12, 1872.)

Cutaneous Absorption in the Bath.—The question of fact, whether the skin in a bath absorbs water and the mineral elements contained in it, has been brought up anew in the Academy of Sciences by M. Jamin (*Jour. de Med. et Chir. Pratiques*).

It would seem, if there is any real absorption, that its amount might be ascertained by increase in the weight of the body. This is not so easy, after all, for the exact weight is constantly varying, from hour to hour. The waste is incessant, while the repair is periodical.

MM. Jamin and De Laurès repeated, at Néris, the old experiments of Sanctorius.

To fully comprehend these experiments, it should be borne in mind that a man in good condition takes 4000 grammes (about one hundred and thirty ounces) of nourishment a day, of which he expels 1500 grammes (about fifty ounces) of refuse. The remaining 2500 grammes (or eighty ounces), after being assimilated, pass off by the lungs and the skin in the course of the twenty-four hours. This is a waste of about 100 grammes (rather more than thirty ounces) per hour.

But this loss is not uniform; it attains to 125 grammes after dinner, and diminishes gradually through the night to breakfast of next morning—becoming, according to MM. Jamin and De Laurès, about 80 grammes between six and seven o'clock in the morning. After breakfast the waste is increased—being lessened by rest and augmented by exercise—and may attain to 340 grammes during a midday's walk in the sun.

This loss is due to two causes, respiration and evaporation from the entire surface of the body. According to Lavoisier and Séguin, the proportionate rate of waste from these causes is as one for respiration to two by evaporation.

According to Durieu, every one preserves his weight without loss in a bath of moderate temperature; gaining by absorption if the temperature is reduced; losing, on the contrary, if the temperature is raised; and this loss increases very rapidly when the water is heated to 95° to 115° Fahr.

The experiments of MM. Jamin and De Laurès, at Néris, confirmed in all points the conclusions of Durieu. A subject whose loss from six to seven o'clock in the morning was found to have a mean of 79 grammes, then entered a bath of 95° and remained there till nine o'clock, two hours. His loss was at the rate of 700 to 800 grammes per hour. At ten o'clock, an hour after leaving the bath, he was again weighed, and the loss was found to be only 50·25 grammes. Although there had been no absorption in this case, it does not invalidate the therapeutic value of baths of mineral waters at a high temperature.

A new point on which M. Jamin insists is, that after a bath the loss is much less than before, often nothing, and, in one experiment, in four bathers he found a very slight increase of weight. In fact, it is noticeable that the weight remains almost stationary some time after a long immersion in hot water.

M. Jamin's explanation does not fully solve the question of cutaneous absorption. He takes too little account of external pressure, on which the amount of exhalation greatly depends, in the water as well as in the air.

Thus, the qualities of a bath reside, in a measure, in its own temperature and density; nevertheless it is necessary to keep in view its chemical composition, which may influence the quality of the perspiration, excite the skin and nervous system, and, in modifying the surrounding atmosphere, introduce mineral elements into the air tubes and the lungs.

The Inventor of Spectacles.—On a tombstone in Florence is this inscription:—"Here lies Salvino Armato d'Armati, of Florence, the inventor of spectacles. May God pardon his sins. The year 1318."

Medical Miscellany.

LECTURE BY DR. BROWN-SEQUARD.—The members of the medical profession, in Boston and vicinity, will be glad to learn that Dr. Brown-Séquard, who has lately returned to America, is to afford them an opportunity of hearing a lecture from him. The lecture, which will be free, will be delivered on the evening of Saturday next, Oct. 5th, at 7½ o'clock, in the lecture-room of the Boston Society of Natural History, corner of Boylston and Berkeley Sts. The subject will be The Origin and Signification of the Symptoms of Brain Disease, and will be illustrated by experiments.

Dr. W. L. Richardson has been appointed one of the Visiting Physicians of the Children's Hospital, in place of Dr. Cotting, resigned.

MASSACHUSETTS GENERAL HOSPITAL.—Dr. Shaw, late Resident Physician of the Massachusetts General Hospital, has removed to No. 28 Marlboro' St., and Dr. Norton Folsom has taken his place at the hospital. Dr. James J. Putnam has been appointed Electrician to the Hospital.

A STATED MEETING of the Suffolk District Medical Society was held Wednesday afternoon, September 18th. The most important item of business transacted was the amendment of the By-Laws of the Society, so that the stated meetings heretofore held in April and September will, in future, occur in March and October, in connection with regular social meetings of the Society. Various committees were appointed, the secretaries of sections who served last year were re-appointed, and minor matters of routine business were attended to.

DR. EVORY KENNEDY, of Dublin, the eminent obstetrician, it is stated is to run for Parliament at the coming election in Derry.

CHLOROFORM AMONG THE CHINESE.—According to a reputed discovery by M. Stanislaus Julien, it appears that as far back as the third century of our era, the Chinese were in possession of an anæsthetic agent which they employed in the same manner as we use chloroform and ether for producing insensibility during surgical operations. A description of this was discovered by M. Julien in a work preserved in the "Bibliotheque Nationale"—called "Kou-kin-i-tong," or a "General Collection of Ancient and Modern Medicines"—which appears to have been published in the sixteenth century. In a biographical notice of Hoa-tho—who flourished under the dynasty of Wei, between the years 220 and 230 of our era—it is stated that he gave the sick a preparation of chanvre (Ma-yo), who in a few moments became as insensible as one plunged in drunkenness or deprived of life; then, according to the case, he made incisions, amputations and the like. After a certain number of days the patient found himself re-established, without having experienced during the operation the slightest pain. It appears from the biography of Han that this chanvre was prepared by boiling and distillation.

THE posting of placards of quack medicines has been prohibited in the streets of Chicago.

BOOKS RECEIVED.—The Pathology of the Teeth, with special reference to their Anatomy and Physiology. By Carl Wedl, M.D., Professor of Histology in the University of Vienna, &c. Translated by W. E. Boardman, M.D., with notes by T. B. Hitchcock, M.D., D.M.D., Professor of Dental Pathology and Therapeutics in Harvard University. Philadelphia: Lindsay & Blakiston. 1872. Pp. 470. (From A. Williams & Co.)—The Science and Practice of Medicine. By William Aitken, M.D., Edinburgh, Professor of Pathology in the Army Medical School. Third American from the Sixth London Edition, with additions by Meredith Clymer, M.D. Penn. Philadelphia: Lindsay & Blakiston. 1872. Two Volumes. Pp. 1056 and 962. (From A. Williams & Co.)—On some Affections of the Liver and Intestinal Canal, &c. By S. H. Ward, M.D., London, F.R.C.P., &c. Philadelphia: Lindsay & Blakiston. 1872. Pp. 260. (From A. Williams & Co.)—Epidemic Cerebro-spinal Meningitis, with an Appendix on the present Epidemic in the city of New York. By Meredith Clymer, M.D. Penn. etc., Philadelphia: Lindsay & Blakiston. 1872. Pp. 59. (From A. Williams & Co.)—Practical Lessons in Nature, and Treatment of the Affections Produced by Contagious Diseases, with an account of the Primary Syphilitic poison and of its Communicability, &c. By John Morgan, M.D., F.R.C.S. &c., Philadelphia: J. B. Lippincott & Co. 1872. Pp. 336. (From James Campbell.)—Evolution of Life. By Henry C. Chapman, M.D., Philadelphia: 1872. Pp. 193. (From James Campbell.)

Deaths in seventeen Cities and Towns of Massachusetts, for the week ending Sept. 21, 1872.

Cities and Towns.	*No. of Deaths.*		
Boston	153	Fitchburg	5
Charlestown	20	Taunton	5
Worcester	24	Newburyport	6
Lowell	20	Somerville	18
Milford	5	Haverhill	6
Chelsea	7	Holyoke	5
Cambridge	30		350
Salem	10	*Prevalent Diseases.*	
Lawrence	11	Cholera Infantum	73
Springfield	3	Consumption	47
Lynn	22	Dysentery & Diarrhœa	19
		Typhoid Fever	19

Seven deaths from smallpox occurred in Boston.

GEORGE DERBY, M.D.,
Secretary of State Board of Health.

DEATHS IN BOSTON for the week ending Saturday, September 28, 147. Males, 84; females, 63. Accident, 5—apoplexy, 1—aneurism, 1—amputation of leg, 1—inflammation of the bowels, 2—bronchitis, 1—inflammation of the brain, 1—congestion of the brain, 1—disease of the brain, 4—cancrum oris, 1—cancer, 2—cholera infantum, 21—consumption, 23—convulsions, 4—croup, 1—debility, 2—diarrhœa, 6—dropsy of brain, 2—dysentery, 2—epilepsy, 2—erysipelas, 1—fever, 3—typhoid fever, 8—gangrene, 1—gastritis, 2—disease of the heart, 3—disease of the kidneys, 1—disease of the liver, 1—congestion of the lungs, 1—inflammation of the lungs, 4—marasmus, 5—measles, 1—old age, 1—paralysis, 2—pertonitis, 1—premature birth, 3—scalded, 1—smallpox, 18—sunstroke, 1—tonsillitis, 1—unknown, 5.

Under 5 years of age, 66—between 5 and 20 years, 18—between 20 and 40 years, 33—between 40 and 60 years, 18—above 60 years, 12. Born in the United States, 102—Ireland, 24—other places, 21.